DOCUMENT RESUME

ED 148 898 TM 006 869

AUTHOR Aldinger, Beverly A.; And Others
TITLE Weighting the Cases as a Method for Handling Attrition in the Mountain-Plains Career Education Program.
INSTITUTION Abt Associates, Inc. Cambridge, Mass.
PUB DATE Apr 77
NOTE 32p.; Paper presented at the Annual Meeting of the American Educational Research Association (61st, New York, New York, April 4-8, 1977) ; Best copy available

EDRS PRICE MF-$0.83 HC-$2.06 Plus Postage.
DESCRIPTORS Adults; American Indians; *Attrition (Research Studies); Career Education; Caucasians; *Data Analysis; Depressed Areas (Geographic); *Economically Disadvantaged; Employment Programs; *Family Programs; Interviews; Longitudinal Studies; Quality of Life; *Research Methodology; Research Problems; *Rural Family; Social Services; Statistical Bias
IDENTIFIERS *Mountain Plains Program

ABSTRACT

The problem of attrition often arises in longitudinal studies when the researcher is unable to gather follow-up information for a sizeable percentage of the study participants at one or more of the planned follow-up stages. Since non-respondents frequently differ in important ways from respondents, the findings based solely on the respondents are not likely to be representative of the total participant population. After investigation of numerous attrition-adjustment procedures, a cross-tabulation approach was utilized, weighting respondents proportionately to non-respondents with the same basic characteristics. The Mountain-Plains Career Education and Employment Program--a residential, family-centered program for multiple-problem disadvantaged, rural, poverty-level families--is used to illustrate this general approach to data analysis. (Author/MV)

****
* Documents acquired by ERIC include many informal unpublished *
* materials not available from other sources. ERIC makes every effort *
* to obtain the best copy available. Nevertheless, items of marginal *
* reproducibility are often encountered and this affects the quality *
* of the microfiche and hardcopy reproductions ERIC makes available *
* via the ERIC Document Reproduction Service (EDRS). EDRS is not *
* responsible for the quality of the original document. Reproductions *
* supplied by EDRS are the best that can be made from the original. *
****

ED148898

TM006 869

U.S. DEPARTMENT OF HEALTH, EDUCATION & WELFARE
NATIONAL INSTITUTE OF EDUCATION

THIS DOCUMENT HAS BEEN REPRODUCED EXACTLY AS RECEIVED FROM THE PERSON OR ORGANIZATION ORIGINATING IT. POINTS OF VIEW OR OPINIONS STATED DO NOT NECESSARILY REPRESENT OFFICIAL NATIONAL INSTITUTE OF EDUCATION POSITION OR POLICY

Weighting the Cases as a Method for Handling Attrition in the Mountain-Plains Career Education Program*

Beverly A. Aldinger

Richard L. Bale

Jay Magidson

Abt Associates, Inc.

"PERMISSION TO REPRODUCE THIS MATERIAL HAS BEEN GRANTED BY

Beverly A. Aldinger

TO THE EDUCATIONAL RESOURCES INFORMATION CENTER (ERIC) AND USERS OF THE ERIC SYSTEM."

BEST COPY AVAILABLE

*The authors are grateful to Dr. John Creager from the American Council on Education for helpful comments.

Weighting the Cases as a Method for Handling Attrition in the Mountain-Plains Career Education Program

BEVERLY A. ALDINGER
RICHARD L. BALE
JAY MAGIDSON
ABT ASSOCIATES, INC.

ABSTRACT

The problem of attrition often arises in longitudinal studies when the researcher is unable to gather follow-up information for a sizeable percentage of the study participants at one or more of the planned follow-up stages. Since non-respondents frequently differ in important ways from respondents, the findings based solely on the respondents are not likely to be representative of the total participant population.

After investigation of numerous attirtion-adjustment procedures, a cross-tabuation approach was utilized, weighting respondents proportionately to non-respondents with the same basic characteristics. Data from the Mountain-Plains Career Education Program is used to illustrate this general approach.

1. SCOPE OF THE PROBLEM

A common problem often encountered in longitudinal studies is that of attrition. The longer the span of time that a study requires subjects to be interviewed, the more likely it is that a substantial number of respondents will be lost from the data stream.

For example, the original experimental design of the Mountain Plains Career Education and Employment Program called for randomized assignment of eligible families to the program or a control group. In addition to being interviewed prior to entering the program, all families were scheduled for 6,12,18, and 24 month follow-up interviews after program exit (due to completion or drop-out).[1]

For a variety of reasons, only a small fraction of families received all four follow-up interviews. Some families were interviewed only once at 6 month follow-up while others at only 18 months. Still others received two of the four scheduled interviews. Well over one third of all participating families were not interviewed at all after exiting from the program.

Because of the nature of the program participants - multiproblem families, and other factors, the attrition rate experienced in the Mountain Plains program is probably more severe than that encountered in most longitudinal studies. It is precisely

---

[1] Based on the average program duration of 8 months, an estimated exit date of 8 months following the initial interview was used for scheduling interviews with the control families.

this reason that makes it a good example for illustrating various approaches for dealing with attrition. The problem was too severe to ignore, yet not severe enough to warrent "throwing in the towel" and not conducting any evaluation.

This paper describes a theoretical framework which we, the evaluators, used in thinking about the attrition problem and presents some relatively simple comparisons of analyses based upon three different approaches of handling the situation. In the next two sections we provide some descriptive and historical information on the Mountain Plains program and describe some design complexities. In section four we give a theoretical framework and describe our approach. Results are summarized in section 5. We conclude with some suggestions for future research.

The scope of this paper is limited to that aspect of the evaluation which is summative in nature. That is, it presents comparisons of all participants (including resignees) with all control families, and addresses only research questions inquiring about the overall effect of the program. The population consists of all families scheduled to receive four follow-up interviews, who exited from the program as of July 1, 1974.[2] Readers wishing further details of the evaluation including a summary and discussion of results are referred to Bale and Molitor (1976).

---

[2]The exclusion of those families who exited the program too late to receive all four follow-up interviews by the date of the evaluation reduces the size of the attrition problem somewhat.

2. THE MOUNTAIN PLAINS PROGRAM: SOME DESIGN COMPLEXITIES

In July 1974 Abt Associates, Inc. was awarded a 30-month contract by the National Institute of Education (Contract NIE-C-74-1047) to conduct a longitudinal follow-up study of participants (and a control group) in the Family Education and Employment Program (formerly known as the Mountain-Plains Education and Employment Program, Inc.). The primary purpose of the study was to determine the sucess of the program in enhancing the quality of life of the participants in five major dimensions: economic well-being, well-being in the world of work, well-being in the community, family well-being, and personal (affective) well-being.

The Mountain Plains program is a residential, family-centered career education and employment program oriented to the needs of the multi-problem, disadvantaged families caught in the poverty cycle. It offers an integrated program of services to the entire family, based on its fundamental assumption that in order to break the cycle of poverty it is necessary to address problems and needs simultaneously at the family level. The program generally takes from 6 to 12 months to complete, depending upon the particular type(s) of courses in which the family enrolls.

Since its completion in February of 1972, the Mountain Plains program has served over 1200 disadvantaged families from the rural areas of Idaho, Montana, Nebraska, North Dakota, South Dakota, and Wyoming, a sparsely populated region that

encompasses nearly one-fifth of the United States. Eighty percent of the families served by the program are White, and the majority of the non-White families are Indian. The 1200 families served represents over 4500 individuals.

The outcome objectives of the Mountain-Plains program are broad-ranging. The global goal can be stated as enhancement of the "quality of life" of program participants. This goal incorporates the subgoals of improving the quality of life in the five domains mentioned previously.

A major complexity in the research design was introduced by the fact that the Mountain-Plains program operated on an open-entry/open-exit basis. A small group of families entered the program on a weekly basis, and each family left the program after completing its individually tailored program of instrution (if they did not resign prematurely). This means that opposed to the (research) conveniences offered by the tradional schools, there were no "cohorts" of students at Mountain-Plains. Rather there was a continuous stream of entries into and exits out of the program. This continuous stream of entries and exits, when considered in the context of follow-up interviews intended to assess program impacts at six-month intervals after exit and the length of time over which the data for the Follow-Up Study were collected (nearly four years), almost assures that program impacts as manifested at each follow-up point (6, 12, 18, and 24) are confounded both with fluctuations in environmental (exogenous) factors and with the developmental stages of Mountain-Plains as an organization. This occurs because

of the two different types of "time" implied in the design -- program time and calendar time. The design was focused on assessing program impacts specifically at 6, 12, 18, and 24 months post-exit from the program. Because of the open-entry/ open-exit policy, this means that some 6 month interviews took place in August 1972, some in April 1974, and some in December 1975, etc. The same dispersion across calendar time is true for 12, 18, and 24 month follow-up interviews, as well as for the pre-Center screening interviews. The analytic problem raised by this aspect of the design is how to render comparable for purposes of analysis, say, 6 month interviews conducted at widely separated points in calendar time.

A second complexity, and one particularly important in assessing the efficacy of the program, was introduced by the fact that many experimental families left the program before completing their course of study (families we refer to as "non-completer"). Non-completers stayed in the program for widely varying lengths of time (from a few days to several months) before leaving, and therefore did not constitute a homogeneous group for purposes of assessing program impacts. This is particularly the case if program impacts are correlated with length of stay in the program, regardless of whether or not a certificate of graduation was earned. There are, therefore, three immdeiately identifiable groups involved in the experimental design: a control group, an experimental group that can be defined as having a "complete" treatment (completers), and a experimental group that has not received the

"complete" treatment (non-completers). For the purpose of this paper we distinguish only between the participants (completers and non-completers) and the control families.

A third complexity stems from the fact that for a variety of reasons a considerable number of experimentals -- both completers and non-completers -- and controls did not receive all four follow-up interviews called for in the original design. Table 1 presents the distribution of reasons for terminations from the follow-up interview stream by experimental status. In addition to the terminations for these reasons, considerable numbers of experimental and control group families are missing one or more interviews for one or more of the following reasons:

> Because AAI data collection ended before they had received the full compliment of interviews;
>
> Because a change in the scope of work specified in the AAI evaluation contract precluded the tracking of some families; or
>
> Because for one reason or another, the family simply could not be reached.

Both Abt Associates, Inc. and the Mountain-Plains Economic and Development Program, Inc. had responsibility for conducting follow-up interviews.

3. THE NATURE OF ATTRITION PROBLEMS ENCOUNTERED

As hinted in the last section, attempting to conduct a sophisticated longitudinal design in a natural setting is no simple task. Keeping track of the families, scheduling their interviews, locating them close to the interview date, and

TABLE 1

TERMINATIONS FROM THE FOLLOW-UP STUDY

	# Cases Terminated		
	Part.	Controls	Totals
Reason for Termination			
Lost	93	10	103
Refused	17	4	21
Institutionalized	0	2	2
Died	0	2	2
Moved out of MP area	72	16	88
Back on center	2	0	2
Respondent broke appointment	3	1	4
Respondent always on road	1	0	1
Dangerous situation	2	0	2
Because of hostility during initial AAI interview family was terminated	2	0	2
Tracking was initiated but interview not completed before all interview protocols were ordered to return	50	6	56
Respondent unable to deal with interviewer due to alcohol/drugs	1	0	1
Respondent moved so far from an interviewer, but still in MP area	0	1	1
Tracking was initiated and some leads were established but family could not located before interviewer was due back in house	2	0	2

TABLE 1

(continued)

Family was lost between MP administration and AAI administrated intervirews (usually address provided by MP is old or insufficient)	13	13	26
Letter was sent to family but interview protocol was not sent to field because of temporary stop-order placed on field operations	27	2	29
Interview not completed because of problems with AAI interviews in Wyoming	5	0	5
TOTALS	290	57	347

actually conducting an interview are some of the required steps. All of these provide an opportunity for "losing" a family from field operations. Some families were permanently lost from the study at various points in the stream of follow-up interviews. Other families were missing only one or more of the expected interviews. There are several aspects of the Mountain-Plains Follow-Up Study which make our attrition problems especially severe. For example, the very nature of the participants, that of multi-problem families, definitely influenced the rate of attrition as shown by Table 1. Some families were lost from field operations when emotional problems, abuse of alcohol or other drugs, or criminal activities resulted in institutionalization. A few families were dropped from the study when we felt it was too dangerous to send an interviewer into the home. In other instances the interviewer would report that the respondent was incapable of responding to our interview, usually because of alcohol or other drugs.

The first three years of the follow-up data collection were the responsibility of the Mountain-Plains Economic and Development Program, Inc. During this time less than half of all follow-up interviews were actually conducted. While some families were lost permantly from field operations during this time, other families were found when Abt Associates, Inc. assumed responsibility in February 1975.

Although Abt Associates did a considerable amount of tracking, the population of families was found to be extremely mobile. It was not unusual to receive <u>several</u> changes of

address for one family over a 6-month interval. Sometimes forwarding addresses were not available for families who seemed to be one step ahead of the creditors. Other times we tracked families outside the Mountain-Plains area; extremely high interview costs prevented us from conducting these follow-up interviews.

Refusals were another problem. Unsatified, disenchanted participants families would occasionally refuse to be interviewed. From time-to-time these families would agree to the interview when it was explained that the interviewer was not associated with the Mountain-Plains Program, but rather another firm interested in evaluating the program. Control families, bitter about not being admitted to the experimental group, also would refuse upon occasion.

Table 2 classifies the participant and control families by their follow-up interview history.[3] It is seen that very few participants received all four follow-up interviews as planned. Some families were interviewed only at six months, etc. as mentioned previously. Notice that a lower percentage of participant families received follow-up interviews than control families. While 37% of the participants were never interviewed again following their initial entry-level interview the corresponding percentage for the control families is only 16%.

Because of the severe attrition rate encountered, the

---

[3]For details on how the control group was partitioned into comparison subgroups for assessing program impacts on completers and resignees see Magidson (1977a)

TABLE 2

FOLLOW-UP INTERVIEW HISTORY FOR FAMILIES EXITING THE MOUNTAIN-PLAINS PROGRAM BY JULY 1, 1974

INTERVIEW HISTORY				ALL PARTICIPANTS	ALL CONTROLS	COMPLETER PARTICIPANTS	COMPLETER CONTROLS	RESIGNEE PARTICIPANTS	RESIGNEE CONTROLS	TOTAL
6	12	18	24							
1	1	1	1	29	28	18	20	11	19	57
1	1	1	0	50	13	36	7	14	10	63
0	1	1	1	8	5	6	4	2	4	13
1	1	0	1	10	5	7	5	3	3	15
1	1	0	0	40	5	28	2	12	4	45
0	1	1	0	2	5	2	4	0	3	7
1	0	1	1	2	0	2	0	0	0	2
0	0	1	1	1	0	1	0	0	0	1
1	0	0	0	59	5	43	3	16	4	64
1	0	1	0	18	0	11	0	7	0	18
1	0	0	1	10	4	4	2	6	4	14
0	1	0	1	8	0	2	0	6	0	8
0	1	0	0	18	16	12	13	6	12	34
0	0	1	0	10	7	5	6	5	3	17
0	0	0	1	2	2	0	1	2	2	4
0	0	0	0	154	18	73	10	81	15	172
			N =	421	113	250	77	171	83	534

summative evaluation would likely suffer in two respects if the analysis proceeded as originally planned. First, the different rate of attrition between the participants and non-participants would likely cause biased estimates of program effect if all interviewed participants were compared to all interviewed control families on the outcome measures recorded on the follow-up interviews. Assuming that the causitive factors associated with a family being interviewed are related to the attrition measures, an estimate of the average outcomes based on only interviewed families would be biased. The difference between these biased estimates for participants and control families would also represent a biased estimate of effect. Moreover (and more important), since the probapility of being interviewed is greater for the control families, the biased estimates for the participants will either be more biased or less biased than the corresponding estimate for the controls. Hence, the estimate of effect will also be differently biased.

The estimate of effect calculated as a simple difference between the average outcome score for the participants and non-participants can not even be interpreted as an unbiased measure of effect for certain classes of families.

Secondly, estimated changes over time would also be biased. We could still plot the average scores at 6, 12, 18, and 24 months for these families who were interviewed. However, these would be mostly based upon different families at each point in time. The changes over time therefore would be likely to reflect differences due to different families rather than true

changes over time associated with each family.

4. THE GENERAL APPROACH

Many alternative methods of analysis were considered at this point. One possibility was to collpase the longitudinal nature of the data base and use the first follow-up interview only, while treating time out of the program as a covariate. We also considered giving respondents who were most similiar to noninterviewed families a larger weight in the analysis, in attempt to unbias the resulting estimates. Such a weighting would need to be performed separately for the participants and non-participants because of the differential nature of the attrition. It would also need to be done separately for 6, 12, 18, and 24 month interviews (unless the data base was collapsed) because of the differential attrition over time. In short, we had a very complex (and messy) problem to deal with.

It was decided to compare three rather simple approaches to give us a general idea of how estimates might differ depending upon the approach taken. First, it was possible that attrition was due to many diverse factors, some of which would bias the estimates in favor of the program while others would bias it in the other direction and collectively they could cancel out. Or it could be that the factors associated with attrition are unrelated to income, self-concept, and employable skills. Thus our first approach would be to Compute estimates at 6, 12, 18, and 24 months based on the unweighted

average value of the interviewed families.

Regarding the second respect in which the analysis might suffer, in order to get an idea of how the effects of the program varied over time we decided to use only those families who received three consecutive interviews, at 6, 12, and 18 months. A comparison of average outcome values for this group with the unweighted means of all interviewed families would also serve as a rough estimate of the extent to which valid inferences regarding changes over time can be made using all interviews.

Finally, we decided to explore a simple weighting procedure to give us an idea what kind of differences we might expect if we weighted.

The idea of weighting is to adjust the follow-up data to the common base of all participants and all controls regardless of the number of families who were interviewed. The key is to select those pre-program factors which differenciate the interviewed from non-interviewed families. In the case that the factors are categorical, they classify families into a number of profiles. Within a given profile, the interviewed families are weighted by the reciprocal of the proportion interviewed.

For example, for a given profile of n families, if 50% of these families were interviewed at (say) 18 months after existing the program, the weight assigned to each of the interviewed families would be 1/.50 or 2. In other words, if the number of interviewed families was equal to the non-interviewed families in a given profile, the interviewed families would be

counted twice; once for themselves and once for the non-interviewed families with the same profile characteristics. Similarily, for a profile in which there were twice as many attrited families as interviewed families, the interviewed families would be weighted by a factor of three; once for themselves and twice more for the non-interviewed families in that profile. Formally, the weight for the j^{th} profile can be defined by:

$$W_j = n_j/f_j$$

where n_j represents the total number in the j^{th} profile and f_j represents the number of these families who were interviewed. For an application of this procedure, see Astin and Panos (1969).

We decided to use only a single dichotomy to define two profiles of families for our preliminary analysis and weight separately for the various categories of the participant (completer and resignees) and control families. (For further information see Footnote 3) The number of states lived in the year preceeding the program was found to be the best single predictor of attrition. We dichotomized this variable at the median to define the profiles. The weights are given in Table 3.

These weights serve two purposes. First, within a given interview period they serve to make the mean outcome variables less biased by attempting to take into account the values of

TABLE 3

CALCULATION OF ATTRITION WEIGHTS

	Number of States Lived in Last Year	n	Number Interviewed				Weights			
			6 mos.	12 mos.	18 mos.	24 mos.	6 mos.	12 mos.	18 mos.	24 mos.
Completers	1	133	90	64	50	27	1.48	2.08	2.66	4.93
	2 or more	117	59	47	31	13	1.98	2.49	3.77	9.00
Completer-Controls	1	15	8	10	10	4	1.875	1.50	1.50	3.75
	2 or more	15	8	12	9	8	1.875	1.25	1.67	1.875
Mid-Controls	1	22	8	16	10	9	2.75	1.375	2.20	2.44
	2 or more	25	15	17	12	11	1.67	1.47	2.08	2.27
Resignee-Controls	1	14	7	8	5	4	2.00	1.75	2.80	3.50
	2 or more	22	14	14	12	8	1.57	1.57	1.83	2.75
Resignees	1	87	37	32	22	18	2.35	2.72	3.95	4.83
	2 or more	84	32	22	17	12	2.625	3.82	4.94	7.00
Total		534	278	242	178	114				

those families who were not interviewed. For example, families completing the program who were interviewed at 6 months had mostly lived in only one state in the previous year (90 one-state families compared to 59 families who lived in more than one state). The non-interviewed completer families, however, mostly lived in more than one state (58 compared with 43). Thus, each interviewed family living in more than one state is counted as 1.98 families whereas each one-state family is weighted only as 1.48.[4]

Secondly, the weights serve to equate each interview period to the same base. Changes in means over time thus become more meaningful. Ideally, it might be expected that a plot of the weighted means over time would appear to show more of a smooth upward pattern than a plot of the unweighted means. The pattern might be expected to also appear to be similar to a plot of (unweighted) means for the subset of families receiving all four follow-up interviews.

5. RESULTS

Figure 1 presents the Participant-Control comparisons of means on average monthly employment income using the three different methods to compute their means.

As a test of whether the attrition weights are in fact

---

[4] These weights were standardized prior to the analysis in order to keep the degrees of freedom constant. The six month weights were reduced by a factor of 278/534; the 12 month weights by 242/534, etc.

adjusting to a common base, we compare the weighted means with the means obtained from the repeated-measures class of families. Recall that this latter set of families are the subset of families who received all three follow-up interviews. (the difference in sample sizes over time for this group is due to item non-responses or a change in classification of Man of House over the period of this study.)

The repeated-measure families in Figure 1 show a very smooth upward concave trend, the gap between the participants and controls growing slightly over time. One might expect to find a rather smooth curve when there is a common base of families if the economy in general remained somewhat constant over this period. On the other hand, large jumps in the curve might be expected if the subset of families upon which the different means were based changed substantially over time to very different families.

The attrition weighted means also form a reasonably smooth upward trend and hence are supportive of the hypothesis that they do in fact adjust the unweighted means to a common base. On the other hand, the fact that the weighted means are not much different than the unweighted means lends doubt to the assumption that the attrited families are substantially different with respect to those factors relevant to the outcomes from the families interviewed.

Next we turn our attention to the number of weeks unemployed. The pattern exhibited by the weighted means again is similar to that of the repeated-measures families but also

extremely similar to the unweighted means.

Also notice that the differences between the participants and controls is largest with respect to the repeated-measured families. This might be related to the possibility that the repeated-measured families are the most stable families (since they are easiest to locate for an interview) and hence possibly most susceptible to the influence of the program. If this is true, the repeated-measures families constitute a biased subset of all families. If so, either the weighted or unweighted means associated with all families would be more representative of all participants.

Participant-Control comparisons regarding Occupational Status are presented in Figure 3. The participants and controls start out at pre-program very similar on occupational status and move quickly apart after six months. (The difference was most apparent for those completing the program.) Each of the three methods of summarizing the means shows this difference to persist throughout the 18 months following the program. Again, notice that the differences between the weighted and unweighted means of all respondents are slight.

6. SOME CONCLUSIONS

Plots based on the repeated-measures families, defined as those families for which we had three consecutive interviews, yield positive estimates of effect while the other two plots are somewhat ambiguous to interpret. One possibility

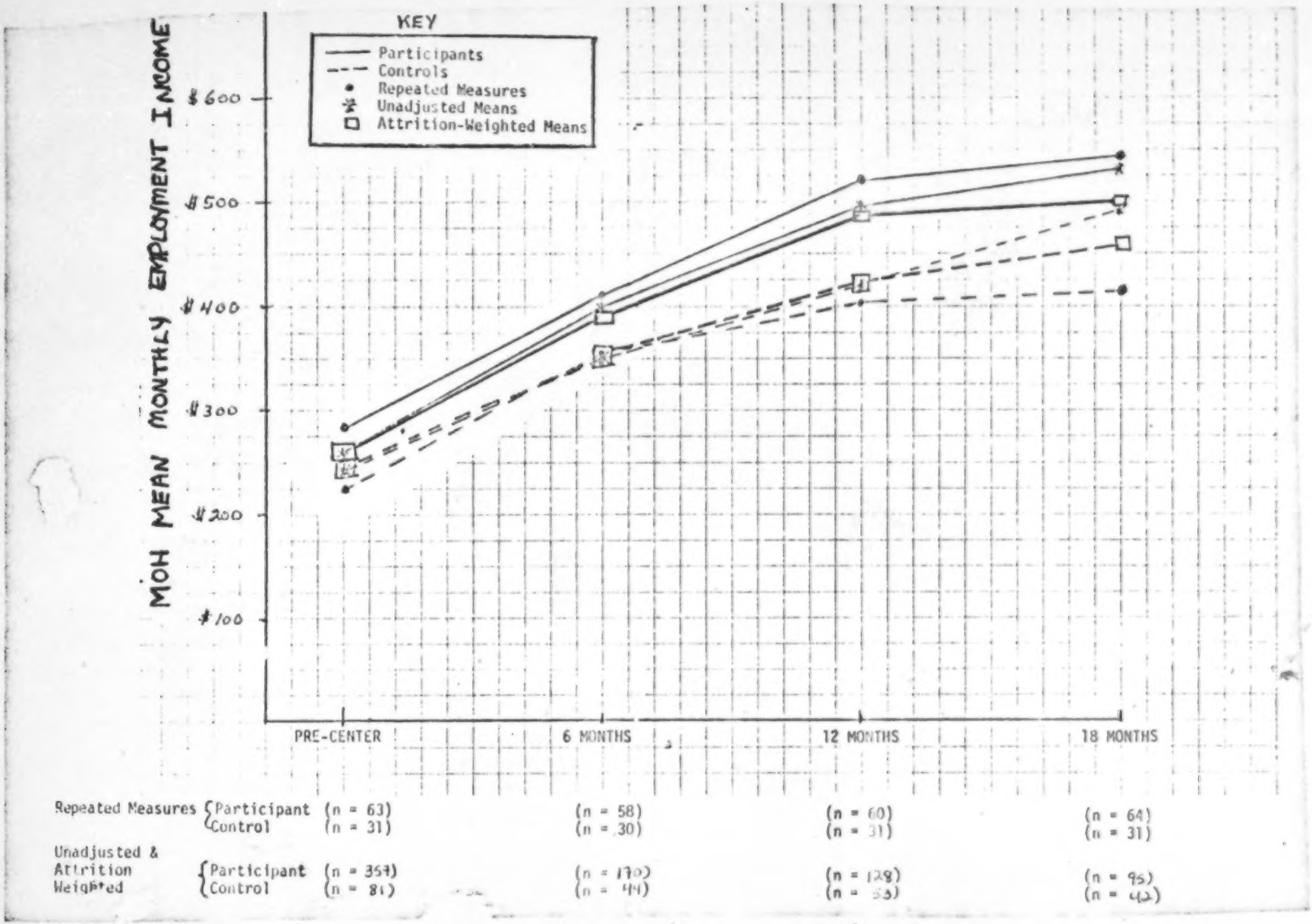
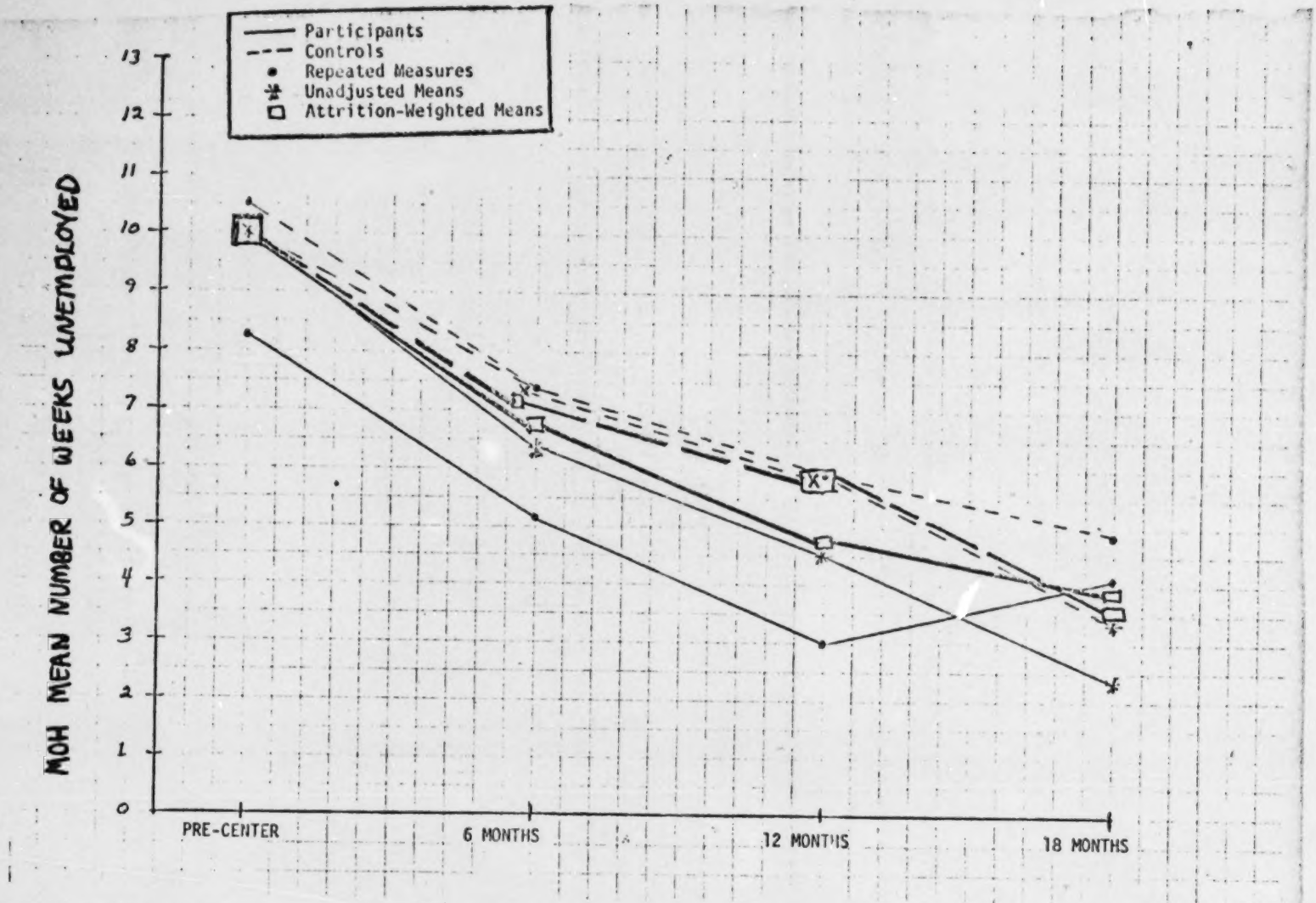
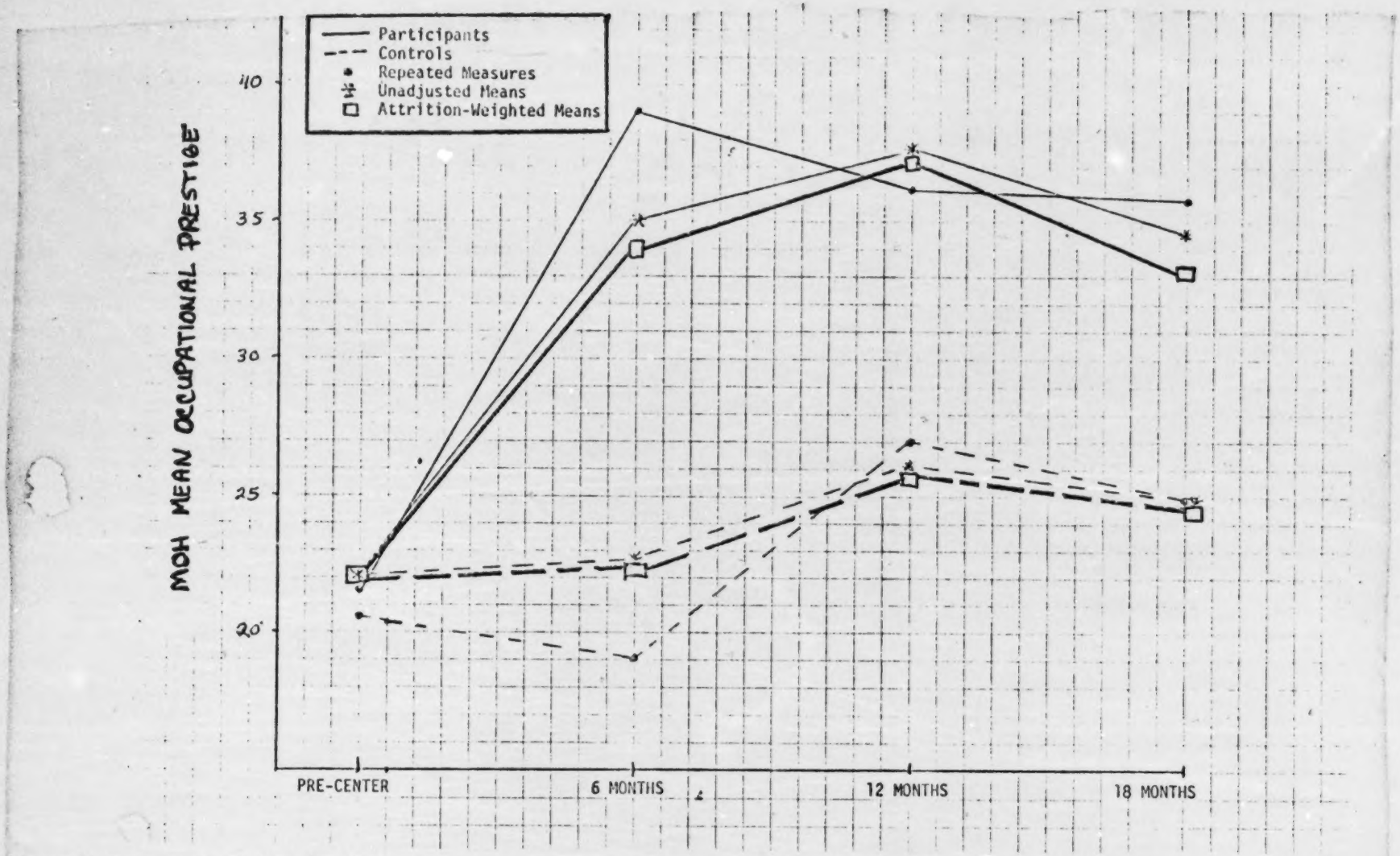

KEY
Participants
Controls
Repeated Measures
Unadjusted Means
Attrition-Weighted Means
MOH MEAN MONTHLY EMPLOYMENT INCOME
$600
$500
$400
$300
$200
$100
PRE-CENTER
6 MONTHS
12 MONTHS
18 MONTHS
Repeated Measures Participant (n = 63) (n = 58) (n = 60) (n = 64)
Control (n = 31) (n = 30) (n = 31) (n = 31)
Unadjusted & Attrition Weighted Participant (n = 359) (n = 170) (n = 128) (n = 95)
Control (n = 81) (n = 44) (n = 33) (n = 42)

FIGURE 1



Repeated Measures	Participant (n=63)	(n=63)	(n=63)	(n=65)
	Control (n=31)	(n=33)	(n=34)	(n=33)
Unadjusted & Attrition Weighted	Participant (n=357)	(n=138)	(n=134)	(n=97)
	Control (n=91)	(n=43)	(n=54)	(n=44)

FIGURE 2



Repeated Measures	Participant	(n=62)	(n=63)	(n=62)	(n=64)
	Control	(n=31)	(n=32)	(n=32)	(n=33)
Unadjusted & Attrition Weighted	Participant	(n= 367)	(n= 138)	(n= [illegible])	(n= 9[illegible])
	Control	(n= 81)	(n= 46)	(n= 5[illegible])	(n= 44)

FIGURE 3

for these differences between the methods is that the repeated-measures families are a biased usbset of all families, perhaps the most stable of the families and hence easy to locate for interviews. It is possible that the program did have positive effects on these families. It is also possible that the repeated-measures control families are not representative of the repeated-measures participant families.

Secondly, the weighted means were very similar to the unweighted means. A possible explanation for this was that the single dichotomy "Number of States Lived in" was not sufficient to capture the relevant differences between the interviewed and non-interviewed families. A general approach for identifying relevant differences and incorporating them into the analysis is discussed in Magidson (1977b, 1977c). (This was not employed in the present study because of cost constraints.)

These are just a couple of many possible interpretations of the data. As has so often been stated, the stronger the experimental design, the more clear cut the interpretation.

REFERENCES

Astin and Panos, 1969. A Four-Year Longitudinal Study of Freshman College Students. Unpublished report, American Council on Education, Washington, D.C.

Bale, Magidson, and Aldinger. The Evaluation of Mountain-Plains Economic & Work Domain Outcomes. Report No. AAI-76-133, a draft working paper, Abt Associates, Inc., Cambridge, Mass.

Bale and Molitor, 1976. Impacts of the Mountain-Plains Program in Five Domains of Quality of Life. Report No. AAI-76-216, a draft working paper, Abt Associates, Inc., Cambridge, Mass.

Magidson, J., 1977a. Partitioning a Control Group to Achieve Appropiate Comparison Subgroups for Assessing Program Impacts on Completers and Non-Completers. Paper presented at the annual meeting of the American Educational Research Association, New York, N.Y.

Magidson, J., 1977b. Towards a Causal Model Approach for Adjusting for the Pre-existing Differences in the Nonequivalent Control Group Situation: A General Alternative to ANCOVA. To be presented at the annual meeting of the American Sociological Association, Chicago, Ill.

Magidson, J., 1977c. Multivariate Methods for Dealing with Unobserved Variables and Measurement Error in Educational Research. Research Proposal AAI 2885, submitted to the National Institute of Education, Washington, D.C.